who, although they are unable to acquire any technical skill in carrying out the operations themselves, should have, nevertheless, an intimate knowledge of the principles which they ought to embody.

We think that more space should have been devoted to that portion of the book which deals with drainage proper. While nearly 100 pages are given to lead-laying and the jointing and bending of pipes, only about twenty pages are devoted to house-drains, and a great part of this is occupied with illustrations of appliances. Not more than one or two pages are given to the subject of cast-iron drains, although they are strongly recommended, and the subject is a very important one. We trust, however, that the author will remedy these deficiencies in the future editions which will doubtless be required to supply the demand for his excellent text-book.

OUR BOOK SHELF.

A Lecture Course of Elementary Chemistry. By H. T. Lilley, M.A. (London: Simpkin, Marshall, Hamilton, Kent and Co., 1892.)

THE abrupt use of chemical terms, and the condensed style adopted by the author in this book, make it evident that it is not specially designed to smooth down the difficulties which confront the unaided learner who approaches chemistry for the first time. It seems rather to be fitted to replace the notes which might be taken on a course of lecture instruction. Regarded in this light it is a useful volume, the knowledge it contains being, in the main, sound and to the point.

It deals with the metals as well as with the non-metals, and dovetailed with the ordinary chemical information are many instances that the author has tried to keep pace with current work, and has attempted to give the student all the important points to be noted in a fairly complete course of elementary chemistry.

A short series of exercises chiefly in chemical arithmetic are given at the end of the book, and a table of contents and an index are supplied.

It would be advisable on p. 53 to say that ordinary sulphur crystallises in the rhombic system. To speak of the crystalline form as an octohedron tends to create an impression common among students, that ordinary sulphur belongs to the cubic system. Fluorine was not made by the electrolysis of liquefied hydrofluoric acid, but of a solution of potassium fluoride in the acid; the pure acid is a non-electrolyte.

It is hardly correct to state that calcium sulphate and hydroxide are the only known examples of solids less soluble in hot than in cold water; calcium isobutyrate and one of the thorium sulphates are additional instances. On p. 98 the flame colorations of potassium and sodium are confused, and brass seems to be omitted in treating of the alloys of copper and zinc. J. W. R.

Longmans' School Geography for North America. By George G. Chisholm and C. H. Leete. (New York: Longmans, Green, and Co.).

IF Mr. Chisholm's well-known geographical text-book was to be extensively used in the United States, it was inevitable that it should be altered in a way which would adapt it to the special needs of American schools. The task was undertaken by Mr. Leete, and he has accomplished it with much skill and judgment. The parts he has rewritten are those relating to America in general, North America, and the United States. To these he gives a prominence which was not necessary or desirable for European students of geography, but which is no doubt essential for learners on the other side of the Atlantic. The plan of Mr. Chisholm's book and the spirit of its execution have both been maintained, and the work ought now to be quite as useful in the New World as it has already been in the Old.

Garden Design and Architects' Gardens. By W Robinson, F.L.S. (London: John Murray, 1892.)

THE author of this book is firmly convinced that to clip and align trees in order that they may "harmonise" with architecture is "barbarous, needless, and inartistic." He is in love with Nature's methods, and would give them in gardens much freer scope than is accorded to them by persons who like best a certain trimness and formality. It is to be regretted, perhaps. that Mr. Robinson deals with the subject in so polemical a temper, but the cause for which he contends is good, and he does excellent service by bringing out prominently what has always been the essential principle of the best and most characteristic kind of English landscape gardening. The value of the essay is greatly increased by a number of well-selected illustrations.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Alleged "Aggressive Mimicry" of *Volucellæ*.

IN the course of a review (NATURE, October 6, 1892, p. 535) of a book, "Animal Coloration," by Mr. Beddard, Mr. Poulton takes occasion to refer to a theory professing to elucidate the resemblance of *Volucellæ* to humble-bees, &c. This reference is occasioned by the suggestion of a counter-hypothesis by Mr. Beddard. The view adopted by Mr. Poulton ("Colours of Animals," 1890, p. 267) is that proposed by Kirby and Spence, and subsequently alluded to by Künckel d'Herculais ("Organ. et Dével. des Volucelles," Paris, 1875) and others; but as Mr. Poulton makes no reference to these authorities he may be assumed to accept the full responsibility. In the place named he says:—"The boldness of these enemies sometimes depends on the perfection of their disguise. Thus the larvæ of flies of the genus *Volucella* live upon the larvæ of bees and wasps. *Volucella bombylans* occurs in two varieties, which prey upon the humble-bees *Bombus muscorum* and *B. lapidarius*, and are respectively like these Hymenoptera. The resemblance is very perfect, and the flies enter the nests to lay their eggs." Mr. Beddard (*l. c.*, p. 225) criticizes the view that the fly resembles the bee that it may with impunity enter the nest, and proposes to look on the presence of the fly's larvæ in the bees' nests as akin to the presence of supposed "pets" in the nests of ants. As Poulton points out, this suggestion leaves the original difficulty of the likeness of the fly to the bee untouched.

Having little interest in either of these speculations, which seem fantastic and premature, it is with reluctance that I take part in the discussion. The case, however, of *V. bombylans* is not only interesting as a striking, and to us in England a most accessible instance of the phenomenon of Mimicry, but as an example of Variation it is almost unique among animals, while among plants perhaps it is paralleled only by Darwin's famous case of the peach and the nectarine. It is besides a case well suited for experiment and close observation. The nests of surface building bees may towards evening be lifted bodily, bees, *Volucellæ*, and all, with a spit of earth, and transferred to a box. This may be taken home and set next morning on a window-sill, when on opening the box the bees will go on with their work for the rest of the summer. If any one seeks an opportunity of honestly trying to get to the bottom of a case of Mimicry, instead of speculating about it at large, he can scarcely find a better case than this. The need for such observations is great, for the account confidently given by Poulton, though according well with his hypotheses, accords with the truth less well.

In these circumstances it may not be out of place to give a brief statement of the facts as they were established by entomologists long ago. The *Volucellæ* are a small group of flies, con-

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taining four British species (Verrall, "Cat. Brit. Dipt.," 1888); of these most if not all resemble various Hymenoptera. The commonest and most remarkable is *V. bombylans*, which may be seen in any English hedgerow on a sunny day in early summer. This fly exhibits the rare condition of existing in two distinct forms in both sexes. The one form is black with a red-tail, in no small degree resembling a small worker of a red-tailed humble-bee, such as *B. lapidarius* L. or *B. Derhamellus* Kirb. The other form has a yellow border to the thorax, yellow hairs on the antero-lateral parts of the abdomen, and a grey tail, to an equal degree resembling a small worker of one of the several yellow-banded humble-bees, *e.g. B. hortorum* L., *B. terrestris* L., or *B. Scrimshiranus* Kirb. Both varieties occur in both sexes and are about equally common. The problem of the evolution of these distinct forms is thus one of the most complex. Some may ask, If the varieties are thus distinct, how are they known to be one species? The evidence of this is (1) that no point of structure can be found to differentiate them, (2) that males of the one variety have been seen coupled with females of the other and *vice versa* (Macquart, "Suites à Buff.," p. 479; Zeller, *Stet. ent. Ztg.*, 1842, p. 66), and lastly (3) that intermediate forms have been found as rarities (Erichson, *Stet. ent. Ztg.*, 1842, p. 115). This evidence may not satisfy all, but as regards Mr. Poulton the identity of the two as one species is not in dispute, for he admits this.

But though the likeness of *V. bombylans* L. and its var. *mystacea* L. (= *plumata* de Geer) to the red-tailed humble-bees and to the yellow-banded humble-bees respectively, is really close, neither these forms nor the less common var. *hæmorrhoidalis* Zt. present any special likeness to *B. muscorum* L., which has a bright brown thorax and a grey abdomen. It is true that Künckel has spoken of a resemblance between the var. *mystacea* and *B. muscorum*, but it is hard to see upon what ground, for indeed it is much as if one were to liken a tabby cat to a fox. As Künckel himself says, the great resemblance of the fly is to the yellow-banded *B. hortorum*.

To return to Mr. Poulton's statement, he says that the two varieties prey upon "*Bombus muscorum* and *B. lapidarius*, and are respectively like these Hymenoptera." These words contain an ambiguity which I cannot believe intentional. But supposing for a moment that one of the varieties *were* like *B. muscorum* (which it is not), the sentence must be taken to mean that each variety preys upon the species of bee which it most resembles, the red-tailed variety on the red-tailed bee and the yellow variety on the other. This is indeed demanded by the hypothesis of "Aggressive Mimicry." In this form the statement is often made, though I never met it elsewhere in print. I invite Mr. Poulton to produce observations in support of that statement. If he will establish it he will do a useful work. When this statement was written I must believe that Mr. Poulton had not read the several authorities on the subject, many of whom relate how both varieties have been reared from the nests of each type of bee, both from the red-tailed and from the yellow-banded (Künckel, p. 58; Drewsen, *Stet. ent. Ztg.*, 1847, p. 211; F. Boie, Kröyer's "Naturh. Tids.," 1838, p. 237). It is still possible that both varieties are born of one mother, and it is possible, too, that each female does her best to choose the nest of a bee like herself, but in support of this hypothesis I know no evidence; and indeed Künckel (p. 58), after considering this possibility, gives it as his opinion that probably the varieties of *V. bombylans* lay indifferently in the nests of all *Bombi*. From the omission of these facts, which to an appreciation of the evidence are vital, we should infer that Mr. Poulton was not acquainted with Künckel's work, were it not that he repeats Künckel's selection of *B. muscorum* as a form resembled by one of the two varieties.

But though Mr. Poulton is wrong in saying that either variety specially resembles *B. muscorum*, he is right in saying that *V. bombylans* preys on this bee's nests, for *both* varieties have been bred from them, even from the same nest (Künckel, p. 58). In my rooms at this moment are several nests of *B. muscorum*, each containing many larvæ of *V. bombylans*, resting for the winter, to emerge in summer, as I hope.

There is then evidence that the two varieties, though they may breed together, yet remain substantially distinct; and that though they respectively resemble different species of bees, they are both found together, not only in nests of bees which they resemble, but also, and in my own experience, more abundantly, in the nests of another bee which they do not resemble.

Mr. Poulton further omits to mention that *V. pellucens*, though in nowise resembling the common wasp, yet lives in its nests, together with *V. inanis* which does resemble a wasp, and *V. zonaria* which is like a hornet (Künckel, pp. 54 and 55). This fact also I commend to Mr. Poulton's ingenuity.

The publication of statements like this of Mr. Poulton's, omitting most salient facts—facts, besides, which, though adverse to his speculations, add a ten-fold interest to the subject—is surely unfortunate. It may be replied that Mr. Poulton's book is of a popular character and does not aim at the completeness of scientific work; but in making choice of evidence, even for popular exposition, it is well to remember that the value of facts is not to be measured by the ease with which they may be momentarily fitted to the sustenance of a facile hypothesis. WILLIAM BATESON.

St. John's College, Cambridge, October 9.

Induction and Deduction.

MISS JONES agitates a question that ought not to settle down without having caused that discussion which its propounding is fit to awaken.

This discussion does not, however, relate to the mutual relations of Induction and Deduction—at least, not as the main topic thereof. It relates to the fragmentary condition of that which is usually referred to and accepted as logic. We are so apt to take it for granted that our so-called logic is tolerably competent and complete as an account of human reasoning in general, that it is of great utility when some one—as Miss Jones does now—raises a question that is adapted to direct our reflections towards some one of the several, perhaps many, gaps that exist in that most important, but too often not understood and misunderstood, branch of science. It is with this specially in view that I have ventured to write this.

In geometry nothing is more usual than to draw a universal conclusion from a case that, to all ordinary ways of apprehension, seems to be a single instance. Indeed, this is one of the cardinal features of geometrical reasoning. Perhaps we might well say that it is the most characteristic feature. It is this feature that the question that Miss Jones agitates ought to call into prominent notice.

She selects for her purpose the case of the isosceles triangle, and asks, How, from premising that the angles at the base of an (one) isosceles triangle are equal to each other, are we logically warranted in concluding that that same equality is true of all isosceles triangles?

That we do thus conclude is known to all, as is also the truth that such a conclusion is a typical one in geometry. Nor have we, nor can we have, the least misgiving as to the rigorous validity of such conclusions.

It would be a digression for me to point out here the essential characteristics of that form of reasoning which, properly speaking, is induction. It is sufficient for my present purpose to remark that true induction is utterly unable to yield us any conclusion that is more than *probable and approximate*.

From these characteristics alone we may know that our geometrical conclusions in view are *not*, as Miss Jones takes them to be, inductive conclusions.

But since our geometrical conclusions are natural and valid, the question still remains, What sort of conclusions are they?

If we propose to call them deductive conclusions, then, when we revert to the array of syllogisms, categorical, hypothetical, disjunctive, dilemmatic, &c., we find none of them, nor any combination of them, that can by any means be made applicable. We have to get not merely from an apparently particular but from an apparently absolutely singular proposition to a universal one. To do this deductively, the body of doctrines and canons, that is usually called logic, confesses itself wholly unable. It lays down as one of its cardinal rules, one that it declares is "founded upon the Laws of Thought," that if any premise is particular, then only a particular conclusion can be drawn.

Nevertheless, I am going to submit that the reasoning under discussion is a true instance, not of induction, but of deduction. I submit that the reasoning that we do actually follow is that which may be formulated thus:—

This isosceles triangle is ANY *isosceles triangle.*

The angles at the base of this isosceles triangle are equal to

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